April 19, 1955

Dear Phil:

Thanks for the O-35 cultures and o5 serum.

Still pending from you: S. virginia

Bill: histories on E. coli's; new subtypes of 055,0111?

me: Serratia's.

Some comments on yours: We'd like to know more about your difficulties with trypaflavine agglutination. Did you get both phases to agglutinate or neither? Had you seen the enclosed paper to work from? There are some fussy conditions (high motility; broth; live or Roccalized cells [formalin n.g.]) but once these were straightened out the results were clear enough. It was odd that H_1^{lw} and H_2^{lw} both behaved like H_1^{e}s, while $H_1^{1,2}$ and $H_2^{1,2}$ both agglutinated.

I had hoped to wait for a new lead, but am discouraged and would like to get the story on the N25-N97 "monophasic" para B's written up. I had in mind a rather technical genetic analysis of the H_1 duplication (starting with H_1^{b} $H_1^{1,2}$ and culminating in such anomolies as 1,2:enx and a:c diphasics), but there is a good deal of historical and serological information that you would be responsible for. Should I go ahead with a first draft on the assumption you will join in coauthorship, and amend and emendeit accordingly? You don't have to commit yourself finally until you see the draft of course; you may not agree with the genetic analysis, but I'd appreciate having your general reaction to the idea anyhow.

You may be interested in the squib Q21. * means that

TM2 ph1 —x abony (/anti b,enx) gave mostly i—enx while
(mixed phases)
ph2 —x 1,2—b.

I am now worried, however, that the ph2 stock may have been that odd monophasic mutant, —:1,2 (Your 53:2034) that cropped up in our TM2 stock, so the difference in the above experiment may not reflect normal phase variation. We're doing it all over again, and with some variety of stocks.

Did Cherry-Davis-Edwards 1953, on phage types of paraB ever reach print? If so, can you spare a reprint?

Q21-22
36.

Yours sincerely,

John